

A BRIEF HISTORY OF OUR UNDERSTANDING OF BEC: FROM BOSE TO BELIAEV

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Summary. — We review how our current ideas about BEC developed in the early period 1925-1965, which had the specific goal of understanding superfluid ^4He . This history is presented by commenting on the key contributions made by Einstein, Fritz London, Tisza, Landau, Bogoliubov, Oliver Penrose and Feynman. We emphasize the emergence of the concept of a macroscopic wavefunction describing the condensate. Starting with the fundamental work of Beliaev in 1957, the period 1957-1965 was a golden era for theoretical studies of interacting Bose-condensed gases. This work provided a sound conceptual basis for understanding the properties of trapped atomic gases which were discovered thirty years later.

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1. – Introduction

I was asked to give an opening lecture about the history of BEC, the subject of this Varenna Summer School. The complete history of BEC is of general interest and someone should write a popular book with the title: *The Quest for BEC: from Bose to Boulder (1925-1995)*. Today I will give you a truncated version of this history, covering mainly the early period 1925 - 1965, from the point of view of a condensed matter theorist. This type of lecture is always scary to give since it involves value-judgements about the

significance of the work of others. However, I think there are several good reasons to review some of the highlights of the early search for BEC and how we think about it:

1. This lecture is like a musical overture, setting out some themes (and conflicts) which will be repeated in the rest of this book.
2. The story is an exciting one, and many of the players were among the greatest physicists of the 20th century. Moreover, many of the key concepts of modern physics (elementary excitations, collective modes, broken symmetry, order parameter, etc) were first introduced in dealing with superfluid ^4He .
3. The history is a complicated one for newcomers to follow (especially for those coming from atomic and laser physics) and perhaps some guidance is needed. It involves great theorists like Landau and London who had completely different visions about what was going on in superfluid ^4He . Current work on trapped Bose gases has analogous conflicting philosophies. In particular, the literature on superfluid ^4He has three major rivers which often have little overlap:
 - BEC and a macroscopic wave function : London \rightarrow
 - Many-body wavefunctions $\Psi(r_1, r_2, \dots, r_N)$ for N atoms: Feynman \rightarrow
 - Phenomenological two-fluid theory based on conservation laws and a quasi-particle description: Landau \rightarrow

When we understand these differences, it is easier to understand the older literature. Moreover, if you don't appreciate this history, you may "repeat it" in trapped gases!

4. A major theme of this lecture will be the emergence of the idea of a macroscopic wavefunction $\Phi(\mathbf{r}, t)$ to describe the unique features which arise in Bose-condensed fluids. The idea started with London in 1938 and by 1965, theorists had developed a fairly complete "conceptual" understanding of Bose-condensed fluids in terms of a broken-symmetry order parameter. This required the development of field-theoretic techniques in the late 1950's. The period 1957-1965 was a "golden period" of the theory of Bose liquids and gases. It is this literature and the associated ideas that have allowed us to make rapid progress on the theory of trapped atomic gases since their dramatic discovery in 1995.

I hope that this sketchy and very personal history of one of the most exciting developments in 20th century physics will encourage professional historians of science to do a more thorough job in the future. The lecture is presented mainly in the form of separated comments on some aspects, *to emphasize that it does not claim to be a systematic study*. As for some general references where you can obtain more detailed information, I would like to suggest:

- (a) Chapter 1 of my book [1] on the modern theory of excitations in interacting Bose-condensed fluids, which includes extensive references.

- (b) The recent scientific biography of Fritz London by Gavroglu [2], who had access to all of London's correspondence and private papers.
- (c) The engaging autobiography of Andronikashvili [3], who gives an insider's day-by-day account of the low temperature group under Landau and Kapitza in Moscow in the period 1935-1960 .
- (d) The Ph.D. thesis of Jurkowitz [4], which examines how the modern theories of superfluidity and superconductivity developed in the 1930's, in the larger context of the development of quantum mechanics.
- (e) The collection of papers by P.W. Anderson [5], with comments by the author. Anderson probably deserves the most credit for his pioneering work (in the period 1957-1963) which clarified the fundamental role of a macroscopic wavefunction in the theory of superfluid ^4He as well as in superconductors. I particularly recommend the reprinted articles on p.144 and p.166 in Ref.[5].

2. – A. Einstein

- Einstein's famous paper [6] was built on a paper by S.N. Bose in 1924, which gave a novel derivation of photon statistics and the Planck distribution. BEC was specifically discussed in the second of three great papers by Einstein on the statistical mechanics of an ideal monatomic gas (written in 1924-25). Pais gives a detailed account of these papers (see p.197 of Ref.[7]).
- Einstein's paper appeared about a year before the development of quantum mechanics. It was the first time anyone referred to or used de Broglie's new idea of *matter waves*. Einstein justified applying Bose's calculation using the argument that if particles were waves, they should obey the same statistics as photons. Schrodinger first heard about de Broglie's idea from reading Einstein's paper. So we might say that Schrodinger's wave equation grew out of Einstein's BEC paper, and not the other way around!
- Einstein's work also preceded the concept of Fermi statistics [8], as well as the division of all particles into two classes (Fermions and Bosons) depending on their net spin (this was cleared up by 1927).
- The 1925 paper was written when Einstein was 46. As far as I know, Einstein never publically commented about this paper or topic again. In the 1930's, Einstein became increasingly interested in the interpretation and completeness of quantum mechanics [9]. In recent years, there has been a resurgence of research on these questions. In this connection, the macroscopic wavefunction associated with a Bose-condensed gas may allow direct tests of certain aspects of quantum mechanics. So we have come around full circle.

- While the properties of normal Bose gases were extensively studied in the decade after Einstein's 1925 paper, nothing much happened concerning BEC until 1938. Apparently a major reason was because George Uhlenbeck (a graduate student of Paul Ehrenfest) criticized Einstein's prediction of a "phase transition" by arguing it would not occur in a finite system (this may remind you of similar criticisms in the last few years in trapped Bose gases!). Second-order phase transitions were not understood yet and Uhlenbeck's criticism was generally accepted (apparently even by Einstein, according to the comments by Uhlenbeck - see p.524 of Ref.[7]). I should add that Uhlenbeck introduced the concept of spin and also became a leading expert in the kinetic theory of interacting gases and statistical mechanics. In particular, Uhlenbeck and his student Uehling gave a very detailed study of the transport coefficients of Fermi and Bose gases as early as 1933 [10].

3. – Fritz London

- London began his University studies studying philosophy then switched to theoretical physics as a graduate student. Is there a message here?
- In the period 1935-37, London (and his brother Heinz London) introduced a new theory of superconductivity based on the idea of a "macroscopic wave-function". These ideas underlie and strongly influenced the development of the modern microscopic theory of BCS, as Bardeen has emphasized many times (in particular, see the epilogue written by Bardeen for Gavroglu's biography of London [2]).
- At a major statistical mechanics conference held in Amsterdam in late 1937, London heard discussions which finally clarified the nature of second-order phase transitions (they require the thermodynamic limit $V \rightarrow \infty$ to be defined in a rigorous way) and also that Uhlenbeck had taken back his criticism of Einstein's 1925 work [11]. This got London interested in Einstein's forgotten paper on BEC.
- At the same time, London also heard rumors of experimental work showing superfluidity in liquid ^4He , below the transition at $T_c \sim 2.17\text{K}$. This transition showed a peak in the specific heat but its physical origin was still a mystery. Indeed, by the early 1930's, there was increasing evidence that below T_c , Helium II (as it had been christened) behaved in strange ways (sudden absence of boiling below T_c [12], infinite thermal conductivity, zero viscosity in small channels, etc). London knew ^4He was a Boson ($S = 0$) and immediately put the two ideas together: *Some sort of BEC was involved in the strange phase transition shown by superfluid ^4He .* As evidence, London noted that Einstein's formula for T_{BEC} gave a good estimate of the observed transition temperature and the specific heat of the ideal gas had a peak at T_{BEC} .
- London told Tisza about this idea and after one restless night [2], Tisza came up with the two-fluid concept, namely that the *Bose condensate acted as a new*

collective degree of freedom, which could move coherently without friction and hence give rise to superfluid behaviour.

- Most of this work was done while London had a temporary research position in Paris at the Institut Henri Poincaré. He was looking for a permanent job outside of Germany, where Hitler's Nazi government was starting to show its real intentions. London finally accepted a professorship in the Department of Chemistry at Duke University in the USA in 1939, where he remained for the rest of his career.
- The year 1938 was a busy period. The above-mentioned statistical mechanics Conference in Amsterdam was in November, 1937. In January, 1938, Kapitza [13] and, independently, Allen and Misener [14], published their key experiments on superfluidity in the same issue of *Nature*. London had his idea in late January, submitted a one page letter to *Nature* in early March, which was duly published in April 9, 1938 [15]. London discussed his idea with Laszlo Tisza in late January, who then submitted a brief note to *Nature* on April 16, which was published on May 21, 1938 [16]. We can all admire the short time needed for publication in those days.
- The first paper by London suggesting the relevance of BEC to liquid ^4He was just a few paragraphs but created considerable interest [2]. More detailed papers were published soon after by both London [17] and Tisza [18]. London was never able to develop his idea about BEC in liquid ^4He in a quantitative manner, and was tremendously frustrated by this over the next decade. He had a vision of the correct theory but the techniques needed to fill in the details had to wait until the late 1950's, when many-body theory was developed. However, modern microscopic theories of superfluidity and superconductivity basically vindicate London's concepts and philosophy [1, 5]. In a strange way, his lack of a microscopic model was an advantage since it forced London to think deeply about the general features a successful theory of superfluidity should have and how these would be mirrored in a "macroscopic" wavefunction. From his point of view, BEC was important because it illustrated how one could produce macroscopic coherence effects directly related to quantum mechanics.
- Earlier in his career, London worked with Schrodinger as a research assistant and shared the latter's idea that the wavefunction in quantum mechanics represented something "real". London's thinking about a macroscopic wavefunction that would describe superfluids and superconductors was clearly influenced by this. However, this approach was not consistent with the developing paradigm of the 1930's. The emphasis on operators in quantum mechanics and the resulting de-emphasis in the Copenhagen interpretation on "pictures" made the tentative ideas of London and Tisza seem very old-fashioned (evidence for this view is discussed in the interesting Ph.D. thesis of Jurkowitz [4]). The theoretical ideas of London really only got "moving" with the later post-war work of Bogoliubov in 1947 [19] and Oliver Penrose in 1951 [20], and finally brought to completion in the early 1960's.

4. – L. Tisza

- Lazlo Tisza worked in Landau’s group in Kharkov during 1935-1937. In the period 1937-1938, he was a visiting research fellow at the Collège de France in Paris, where he interacted with London. Tisza eventually went to MIT in the U.S.A., where he has spent the rest of his career.
- Tisza had nothing but an ideal Bose gas as a microscopic model, yet he developed a “two-fluid hydrodynamics” based on the notion of a superfluid and normal fluid. Tisza’s two long papers published in 1940 on his two-fluid hydrodynamics are very impressive even today [18]. He could explain all the experiments exhibiting superfluidity, usually involving the normal fluid and superfluid moving in opposite directions. Tisza also predicted the existence of a new kind of hydrodynamic oscillation, a temperature wave (later called second sound by Landau).
- London took many years [2] to accept the huge “leap” that Tisza made connecting his BEC idea with superfluid behaviour based on a two-fluid model. Personally, I have been always puzzled by this reluctance. In any event, they remained close friends and corresponded at length [2] on BEC-related questions until London died in 1954.

5. – L.D. Landau

- Landau’s “bombshell” of a paper [21] on superfluid ^4He changed how we think about *all* condensed matter systems.
- While essentially phenomenological, Landau introduced a “new” hydrodynamics to describe low frequency superfluid phenomena based on the motion of two fluids described by ρ_n , ρ_s , \mathbf{v}_n , \mathbf{v}_s , in many ways similar to those developed earlier by Tisza [18].
- In addition, Landau also introduced the novel but powerful idea that the liquid could be described in terms of a “gas of weakly interacting quasiparticles”, with a relatively simple energy spectrum for two kinds of quasiparticles: phonons and rotons. This quasiparticle idea allowed Landau to do quantitative calculations. Landau originally tried to justify his quasiparticle energy spectrum using “quantum hydrodynamics” but this was never convincing. In a later brief addendum [22], Landau introduced the now famous phonon-roton spectrum as a single excitation branch. This modified spectrum was deduced using fits to better thermodynamic data which had become available.
- Landau also introduced the idea of collective modes as distinct from quasiparticles (or elementary excitations). In particular, first and second sound are collective modes in the “gas of quasiparticles”.

- By the early 1950's, the Landau-Khalatnikov (LK) theory had overshadowed the London-Tisza scenario, which still lacked a microscopic model in which interactions between atoms were included. The direct measurement of the phonon-roton spectrum using inelastic neutron scattering [1, 23, 24] dramatically confirmed the correctness of the Landau approach. To this day, the Landau-Khalatnikov theory of superfluid ^4He (as summarized in the classic book by Khalatnikov [25]) is the standard theory used to describe the properties of superfluid ^4He . In his landmark paper, however, Landau never mentioned the fact that ^4He atoms were Bosons, let alone the existence of a Bose condensate. Indeed, the experimental low temperature community still tends to feel BEC cannot play a very fundamental role since the LK theory hardly mentions it! On the other hand, since the 1960's, most theorists have viewed the LK theory as a phenomenological theory whose microscopic basis lies in the existence of a Bose macroscopic order parameter.
- Why did Landau resist the idea of a Bose condensate as being relevant to superfluid ^4He ? This is strange, since it was Landau himself who in 1937 formulated the usefulness of the concept of an *order parameter* to deal with second-order phase transitions. However, even as late as 1949, Landau wrote a blistering attack on Tisza's work in the Physical Review [26]. Overlooking personality conflicts (see Ref.[2]), one interesting explanation is suggested by Jurkowitz [4]. Landau deeply accepted the Copenhagen view of quantum mechanics, as is clear from Chapter 1 of Ref.[27]. In particular, he accepted that the proper procedure was to take a classical description and quantize it by introducing operators for the physical observables. Thus, Landau believed that the correct way of understanding a "quantum" liquid was to *quantize* the standard hydrodynamical theory of a "classical" liquid. This is what he tried to do with his "quantum hydrodynamics" in his 1941 paper - an approach that, as mentioned above, never succeeded. However, it suggests why in the 1940's, Landau felt that it was absolutely wrong to try and develop a theory of a "quantum liquid" starting from a "quantum gas" (which is what the London-Tisza program was trying to do). In modern renormalization group (R.G.) language, one might say that Landau felt these two systems corresponded to two different fixed points.
- More generally, we know that Landau was deeply imbued with the idea that a good theory should be able to give a quantitative explanation of experimental data. He did not like "vague" theories which could not be pushed to a clear experimental conclusion. In any event, as far as I know, Landau never "officially" changed his views of the London program that a theory of superfluid ^4He could be developed "starting" from an ideal Bose gas. However, towards the end of the second of two great papers by Beliaev in 1957, Beliaev writes [28] "It allows one to suppose that the difference between liquid He and a non-ideal Bose gas is only a quantitative one, and that no qualitatively new phenomena arise in the transition from gas to liquid". A few paragraphs later, Beliaev ends his paper by thanking "L.D. Landau for criticism of the results", as much of a "stamp of approval" as one could expect.

6. – N.N. Bogoliubov

- The famous paper by Bogoliubov in 1947 [19] was a real breakthrough, but it took over a decade to be generally understood or accepted by the condensed matter community.
- Bogoliubov’s calculation showed how BEC was *not* much altered by interactions in a weakly interacting dilute Bose gas (WIDBG), something which was not obvious at the time.
- However, Bogoliubov showed that interactions completely altered the long wavelength response of a Bose-condensed gas. The predicted phonon spectrum at low momentum was exactly what was *assumed* by Landau for his quasiparticle dispersion relation and ensured the stability of superfluid flow.
- This paper took up the sputtering program of London and Tisza, and started the developments which led to a “complete” theory by the early 1960’s based on the key role of Bose broken symmetry, and how this modified the dynamics of a Bose-condensed system, be it a gas or liquid (for a review with references, see Ref.[1]). It seems that London never heard of Bogoliubov’s work (there is no mention of it in Ref.[2] or in London’s well known monograph).
- Landau clearly recognized the correctness and importance of Bogoliubov’s results (see Ref.[26]). However, the paper of Bogoliubov was not “understood” until 1957 or so. This was probably mainly due to the use of second quantization (unfamiliar at that time in condensed matter physics) and the use of a number non-conserving approximation. For example, the famous papers of Feynman [29] as well as those by Lee, Yang and Huang (their work is nicely summarized in the *first* edition of Huang’s well known book [30]) in the middle 1950’s make essentially no reference to Bogoliubov’s paper. The appearance of the BCS theory of superconductivity in 1957 [31], however, quickly led to analogies with Bogoliubov’s treatment of a dilute Bose gas [5]. Both theories involve “off-diagonal” symmetry-breaking mean-fields.

7. – O. Penrose and L. Onsager

In liquid ^4He , it is easy to get into the collision - dominated hydrodynamics domain, described by dynamic local thermal equilibrium. Thus all the original experiments in the 1930’s were on low frequency, hydrodynamic phenomena, as were the theories of Tisza and later by Landau. It was the two-fluid domain that was most easily studied, a domain described by the normal fluid and superfluid variables: ρ_n , ρ_s , \mathbf{v}_n , \mathbf{v}_s . An important event was in 1946, when Peshkov in Moscow [32] succeeded in observing second sound as a temperature oscillation and showed that the temperature dependence of the second sound velocity agreed with the prediction of two-fluid hydrodynamics. Direct evidence for BEC (in contrast to superfluidity) was illusive in superfluid ^4He . In contrast, in atomic gases, BEC was immediately observed in the first successful experiment [33], while the

two-fluid hydrodynamic region is difficult to access because of the low density. Only recently have experiments on trapped gases started to probe this interesting two-fluid region [34].

In modern theory, the underlying Bose complex order parameter is

$$\Phi(\mathbf{r}, t) = \sqrt{n_c(\mathbf{r}, t)} e^{i\theta(\mathbf{r}, t)}.$$

The superfluid velocity, defined by $m\mathbf{v}_s(\mathbf{r}, t) = \hbar\nabla\theta(\mathbf{r}, t)$, is easily measured by a variety of experiments. In contrast, the amplitude $\sqrt{n_c}$ is more illusive since it doesn't appear in most measurable properties of superfluid ^4He . What is easily studied is the superfluid density ρ_s , which depends on n_c but in a complicated manner.

In 1956, Penrose and Onsager [35] extended the concept of $\Phi(\mathbf{r})$ to a uniform Bose *liquid* and discussed the associated long-range correlations it implied, building on earlier work by Penrose [20]. They also estimated the value of n_c at $T = 0$ using a ground state wavefunction due to Feynman and found $n_c \simeq 0.08n$ in liquid ^4He . This estimate has not changed much in 40 years! Convincing experimental values of n_c were only obtained in early 1980's, using inelastic neutron scattering to extract the momentum distribution $n_{\mathbf{p}}$ of the ^4He atoms (for reviews, see Ch.4 of Ref.[1] and the article by Sokol in Ref.[36]). These experiments also gave (when carefully analyzed) $n_c \sim 0.1n$ at $T = 0$ and show that $n_c \rightarrow 0$ as $T \rightarrow T_c$.

8. – R.P. Feynman

- Several brilliant papers by Feynman [29] posed the important question: How are the many-body wavefunctions $\Psi_N(r_1, r_2, \dots, r_N)$ for the ground state and lowest excited states of liquid ^4He effected by Bose statistics?
- These papers are essentially variational in nature, but Feynman managed to give the first “microscopic” understanding based on quantum mechanics of the roton part of the quasiparticle spectrum (as first postulated without any explanation by Landau in 1947 [22]).
- Feynman's papers were the beginning of a huge literature on various approximations for the wavefunctions Ψ_N of many-particle quantum states. In particular, one should mention the extensive work of Feenberg and his students and co-workers in developing what is called the “correlated basis function” approach (for a brief review, see Section 9.1 of Ref.[1]). This has been mainly successful in calculating ground-state properties ($T = 0$). However, the role of a Bose condensate in all these theories is obscure. It appears, at best, as a sort of “side effect”. Little contact is made with the field-theoretic approach based on a macroscopic wave-function. Moreover, by concentrating from the beginning on realistic treatment of the hard-core and high density which arise in a liquid, these theories do not appear (so far) to have much relevance to trapped dilute Bose gases.

- Feynman also did fundamental work on vortices and their role in superfluid ^4He . In this connection, Feynman's work inadvertently popularized the idea that somehow "rotons" were fundamentally connected to superfluidity - and moreover, these were related to vortices. This is still a lingering belief in parts of the low temperature community, with not much justification! Needless to say, there are vortices in dilute Bose gases but no "rotons".

9. – Golden era

- The "Golden Period" (1957 - 1965) ends our history of the first phase of the quest for BEC. In this period, many important theorists attacked the interacting Bose-condensed gas problem. It was a hot topic during this period and, in my opinion, the final theoretical edifice is one of the great success stories in theoretical physics [1, 28, 37, 38, 39, 40, 41]. However, until recently it was largely unknown since it was somewhat uncoupled from experiments on liquid ^4He and involved a very complex formalism.
- Out of these efforts came a way of "isolating" the profound role of a condensate on the response functions of a Bose-condensed fluid. This was an amazing accomplishment since it was developed to deal with a Bose liquid. The problems of a liquid had to "separated" off from the effects of Bose condensation. These studies often used a weakly interacting dilute Bose gas (WIDBG) as an illustration. Thus a huge amount of theoretical insight was gained and a whole literature developed about a "fictitious" system, namely a Bose-condensed gas! Today, more than thirty years later, all this "useless" work is very relevant to trapped atomic gases, apart from the need to add a trapping potential. In another article in this book, I give an introductory review of some of this work [42].
- I should mention the important and beautiful work of Lee, Yang and Huang in period 1957-59 on the low temperature properties of a dilute hard-sphere Bose gas [30]. However, their work involved very complex many-body calculations using exact wavefunctions but never dealt with the underlying Bose broken symmetry as Bogoliubov and Beliaev did. The final results are correct but the method of calculation does not give much insight into the physics involved.
- A key development relevant to trapped gases was due to Pitaevskii in the period 1959-61. Many of us now are very familiar with the famous Gross-Pitaevskii equation for $\Phi(\mathbf{r}, t)$ [43]. However, Pitaevskii's real contribution was not this particular equation - rather it lies more in introducing the whole idea of a *macroscopic wavefunction* which could depend on both position and time. Pitaevskii's work, of course, was no doubt inspired by the use of a similar "wavefunction" by Ginzburg and Landau in their pioneering theory of spatially inhomogeneous superconductors [44].

10. – Concluding remarks

A history of scientific ideas often tends to give a misleading impression of a steady march towards the “correct” theory, ignoring the doubts even the original thinkers had about their own ideas. A good example concerns the key role that Bose statistics (and the resulting condensate) plays in the phenomenon of superfluidity of liquid ^4He . It was only in 1949 that the first experiments on liquid ^3He (a Fermi liquid) were reported, showing no evidence for superfluidity down to 1K. It is interesting to read the exchange of letters [2] between London and Tisza about these results and catch their excitement. Clearly, even in 1949, they still had lingering doubts about their key idea, which were finally put to rest by the lack of superfluidity in liquid ^3He . In passing, we might note that these negative results for liquid ^3He were probably a major reason for the renewed emphasis on the role of Bose statistics in superfluid ^4He in the early 1950’s [20, 29].

Finally, beginning with London’s work in 1935-38, I would like to recall that our modern understanding of superfluidity in Bose fluids has gone hand-in-hand with our increasing understanding of superconductivity [5]:

$$\begin{aligned}\Phi(\mathbf{r}) &= \langle \hat{\psi}(\mathbf{r}) \rangle_{sb} \rightarrow \text{Bose order parameter in Bose superfluids [37]} \\ &= \langle \hat{\psi}(\mathbf{r}) \hat{\psi}(\mathbf{r}) \rangle_{sb} \rightarrow \text{Cooper pair amplitude in superconductors [45]}\end{aligned}$$

It was not realized or emphasized in the early years, but the BCS theory [31] really involves a “BEC of Cooper pairs” (see, however, the early paper by Gor’kov [45]). This became clear in 1980’s, when Leggett [46], Nozières [47] and others (for a review, see the article by Randeria in Ref.[36]) pointed out that in the strong coupling limit, the BCS theory reduces to a Bogoliubov theory of a dilute Bose gas composed of small, non-overlapping Cooper pairs. It is exciting that in trapped Fermion gases with an attractive interaction, this BCS phenomenon is once again being addressed (see, for example, Ref.[48]) and that we may be able in the future to study this BCS \rightarrow BEC transition in exquisite detail by changing the value of the scattering length a by working close to a Feshbach resonance [49].

We have come to the end of the *first phase* of BEC studies. From the 1960’s, an experimental search started in earnest for a *pure form* of BEC, without the complications of dealing with a *liquid*. That is, the goal was to find BEC in a *gas*! The major systems that have been studied are (for review of work before 1995, see the review articles in Ref.[36]):

- Optically-excited excitons in semiconductors
- Spin-polarized hydrogen atoms
- Laser-cooled alkali atoms

I leave the writing of this recent history, and especially since 1995, to someone in the audience!

All of these developments would have made Fritz London, one of the most seminal theorists of all time, very happy! His vision and that of Lazlo Tisza has been realized to a degree they could never have predicted sixty years ago.

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During a thirty year career working on the theory of superfluid ^4He and superconductivity, I have enjoyed discussing these questions with many of my colleagues in condensed matter physics. During this Varenna Summer School, I especially appreciated some comments by Lev Pitaevskii about the Landau school. I also acknowledge many stimulating discussions with my new colleagues in atomic and laser physics, who bring a fresh perspective to the study of BEC. I would be happy to hear from anyone who might have useful information to complete or correct the story.

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